zogamy in *Juglans regia.* The large ovule is anatropous. The placenta fills the ovary and frequently fuses with it. From the sides of the placenta develop two peculiar wing-like growths projecting somewhat above the base of the ovule. The pollen tube is strictly intercellular in its growth as in the other Chalazogamia. After the tube has penetrated the stigma and grown through the style, it enters the tissue of the ovary near the canal of the style but without entering its cleft or penetrating the micropyle. During its further growth, in the wall of the ovary, the tube turns to right or left and passing through the wing-like placental growths enters the top of the placenta and from here grows through the chalaza into the nucellus and to the embryo sack. During nearly its entire growth the tube sends out projections and in the chalazal region these become branches which give to the nucellus a veined appearance as if penetrated by a number of distinct pollen tubes. Several of these branches finally reach the embryo sack and surround it on all sides. The author detected the male nucleus, not only in the pollen tube, but also inside the embryo sack. At this time there was in the embryo sack neither an egg apparatus nor a differentiated egg. Besides the antipodal cells, separated from each other by a cellulose membrane, there were only some free nuclei on which devolved the rôle of the female apparatus. These appearances can hardly be explained otherwise than by supposing that the male nucleus fuses with one of the female nuclei to form the egg-cell. In these particulars *Juglans* (also *Corylus*) appears to be related to *Gnetum*, the developmental history of which has been studied critically of late by Geo. Karsten (Cohœn's *Beiträge*, VI). The author now attributes chalazogamy to the inability of the pollen tube to grow through empty spaces, and regards these plants as standing on the threshhold of the angiospermous world. To him they represent transition forms between Gymnosperms in which the pollen tube has an intercellular growth and Angiosperms in which it grows through the mycropyle.—ERWIN F. SMITH.

ZOOLOGY.

Variation in Halicystus octoradiatus.—Among 154 specimens, according to a recent paper in the *Quarterly Journal of Microscopical Science*,[1] Mr. E. T. Brown found 120 normal and 34 abnormal

[1] Vol. XXXVIII, pp. 1–9, Pl. I.

specimens, the normal individual being understood to be one with eight tentacle groups, eight genital bands, eight colleto-cystophores and four well-formed septa. The variations occur in the tentacle groups, the genital bands, and in the number and position of the colleto cystophores. In some cases there is an extra collecto-cystophore, which may be on the edge of the arm of the tentacle group, or within the margin the inner surface of the bell, or even outside the margin. A peculiar variation occurs in the collecto-cystophorus, themselves some of them sometimes bearing a small capitate tentacle. The variation in the genital bands may be due to an apparent splitting of a band or even to a fusion of one band with a ninth or supernumerary one. Two variations in the tentacle groups are interesting. In one individual figured there are seven perfectly normal groups, and one abnormal rather small group occupying a position within the margin upon the inner surface of the bell. Its normal position on the margin is occupied by a large colleto-cystophore with a capitate tentacle. In the other case there are likewise seven normal groups. The eighth is normally placed, but is small. Somewhat outside of it there arises a supernumerary arm bearing an apical group of tentacles and another or proximal group. On each margin of the arm is a colleto-cystophore, thus raising the number of these to ten. It may also be said that the eighth genital band corresponding to the abnormal tentacle group is double.

The author adds that mutilated individuals may reproduce a part that is or is not like the original, and that in some cases these mutilated forms bears a close resemblance to others that are congenitally abnormal. This being the case, it may be said that his observations show that there is considerable room for experiment to determine why the reproduced part is not like the original, and to what extent it may differ.

—F. C. K.

The Role of the Liver in the Anti-coagulating Action of Peptone.[2]—E. Gley and V. Pachon have performed certain experiments that not only demonstrate the correctness of the earlier conclusions of G. Fano, that the anti-coagulating action of peptone injected into the blood of an animal is indirect, but also localize the intermediate agent. The experiments consisted in ligaturing the lymphatic vessels leaving the liver in a dog previously morphined and chloroformed, and then at intervals drawing blood from the left carotid and from the sphenal vein.

[2] Comptes-Rendus de l'Acad. Sci., CXXI, pp. 383–5.

At 3.42 (p. m.), 6 c.c. of blood from the carotid coagulated at 3.43.
At 3.50 to 4 (p. m.) the lymphatics were ligatured.
At 4.09, 8 c.c. of blood coagulated at 4.10.
At 4.22, 5 c.c. of blood coagulated at 4.23.

Then from 4.23–4.26 a solution of 6.5 gr. of peptone was injected into the sphenal vein. At the end of this time blood was drawn at intervals.

7 c.c. drawn at 4.33 coagulated in 1 minute.
8 c.c. drawn at 4.40 coagulated in 1 minute.
8 c.c. drawn at 4.55½ coagulated in 1½ minute.

This clearly shows that by thus preventing the intrahepatic circulation of the lymph, the peptone loses its power of preventing the coagulation of the blood, and consequently that peptone has its usual effect only after having passed through the lymphatics leaving the liver.

—F. C. K.

The Neoformation of Nerve-cells in the Brain of the Ape after a Complete Removal of the Occipital Lobes.[3]—It has commonly been supposed that nerve-cells are not regenerated, and such was the conclusion of G. Marinescu presented to the Société de Biologie in 1894. But physiololgists have observed that animals deprived of the occipital lobes gradually regain the power of coördination of movements and of the recognition of surrounding objects to a degree, at least.

The author, on Aug. 24th, 1895, observed this phenomena, and, upon repeating the operation, was surprised to find the orifices of trepanation closed with a somewhat resisting tissue, and that the space formerly occupied by the occipital lobes had been refilled with a tissue that, upon examination with the rapid Ramon y Cajal Golgi method and by the Erlich hæmatoxylin eosin method, proved to be made of pyramidal nerve-cells and nerve-fibres and neuralgia. The latter was very abundant, while the former were less numerous than in the normal lobes. The growth was not due to the hypertrophy of the anterior lobes, for there was no clear microscopical demarkation between the two parts, and must therefore have been due to neoformation.

He adds that this explains, somewhat, the conflicting results of different observers in cases of incomplete removal of the lobes.

The operation of removal was repeated on the animal, and some three and a half months later the same phenomenon of reviving recognition reappeared.—F. C. K.

The Æstivation of Snails in Southern California.—Like the human genus, snails require rest, days and weeks of solitude, in

fact, the land snail (Helix) withdraws so completely from social intercourse that months are spent in voluntary confinement. So secluded does this little householder become that his door or aperture is closed with one white curtain after another until sometimes one-half a dozen membranous curtains in succession are draped when he has entered into his Nirvanic rest. In this condition his aperture, or outer door, is securely glued to the under surface of a stone, a board, or any substance under which he seeks shelter. In the eastern states he takes his annual *siesta* in winter, this being the period of hibernation.

But in Southern California snails (Helix) differ from their congeners presenting an illustration of the power of environment over natural ininstincts. Instead of going into winter quarters in October and remaining asleep all the winter months, the season of greatest activity of the Southern California snail is during those months.

The reason for this is that the food supply is plentiful in the winter when the warm rains prevail; but, during the summer months the arid condition of the foot-hills, the habitat of these quiet creatures, makes makes the æstivation of snails a necessity, a question of domestic economy, an adjustment of demand and supply. In process of time the necessity for æstivation, rather than hibernation, became a habit. During this period his functions are in a state of coma; digestion, respiration and circulation are imperceptable; he sleeps with all his powers, and his waking is not a voluntary action. Without moisture a snail will rest for years! Dr. R. E. C. Stearns, of the U. S. National Museum, records a rest of six years of one snail from Lower California, *Helix veatchii.*

On March 21, 1890, a few land snails (*Helix traskii* Newcombe), were collected from some of the low foot hills in Los Angeles. These were left in a glass jar on a stand and in the morning the snails had crawled up the wall of the room and were esconsced in one corner of the ceiling, another one had travelled farther in the night and had pre-empted his claim in one corner of the hall ceiling. They were allowed to remain undisturbed in order to study developments. One soon fell down upon the carpet, but the other two remained intact. The household orders were that *Helix traskii* were to be left undis turbed by brush or broom. The summer came and went, antumn followed, winter came on, and still the hermaphrodites remained asleep. No sound of music nor mirth aroused them.

But the rains came on, heavy drenching showers that rushed down the mountains, washed the foot hills, overflowed the zanjas, and all nature was in a dripping condition. During one of these storms, in

January, 1891, the rain made invidious incursions into the hall during the night, and in the morning the snail was found on the carpet. In an hour afterward he was as willing as ever to struggle for existence. He ate heartily of celery, with his little rasping tongue (radula) beset with multitudes of tiny siliceous teeth.

It was not until February 23, that the other snail had been sufficiently overcome by the forces of nature to loosen his epigram enough to descend to the floor. He was then placed in a shallow saucer of water and he assumed his functions as though there had been no state of torpor.

While the house snails were glued to the ceilings, their relatives in a "snailery" in the garden had been aroused to activity by the first rain as it pattered through the screen cover of the snailery, and had been busy housekeeping. As the result, a number of tiny pellucid looking balls were, on January 21, 1891, carefully hidden in the moist earth in the box. These were the eggs of the snails. Time had been lost by the house snails, their siesta, extended beyond the requirements of Nature, had gained them nothing. It was the intention to study all these forms and see if the "house snails" lived any longer for their protracted æstivation, but, alas! for the rapacity of the animal kingdom, slugs, sow bugs, ants and insects from the rosebushes, made war upon the whole snail colony, adults, babies and eggs, and by summer time, the little houses were empty, the tenants were dead.—Mrs. Burton Williamson.

A Careless Writer on Amphiuma.—I have recently read an article in the last number (October, 1895) of the American Journal of Morphology by Mr. Alvin Davison on Amphiuma, which contains such evidence of haste and carelessness as to require early notice. At present I refer principally to his references to my work and my conclusions, but as the errors here are so numerous I cannot suppose that I am the only author favored by misrepresentation,

On page 378 he says, "the number of premaxillo-maxillary teeth is never less than fifty. The number is wrongly stated by Cope as thirty-one." I have recounted the teeth on the specimen which I had in hand when this assertion was written, and I find the number to be exactly as I have stated. Mr. Davison has probably counted the teeth on *both sides* of the skull. One would think that a little scientific imagination would have suggested this explanation of the discrepancy to Mr. Davison.

Our author next describes the squamosal bone of Amphiuma, putting his discoveries as to its shape in italics, as though it had not been often

described and figured before, and then goes on to say that "the bone which Cope has called squamosal in the Cœcilians is quite differently located, being directed forwards and inwards in such a manner as to form part of the orbit, and, therefore, deserves the name of quadratojugal, as some authors have already called it." It is at least amusing to learn that to contribute to the orbit is characteristic of the quadratojugal bone. That is exactly what it never does; and, moreover, the squamosal does not do so in Cœcilia. That the element in question is the bone which is called in Batrachia generally by modern authors the squamosal, there can be no doubt; I prefer however, at present, to call it supratemporal. Mr. Davison's osteology is here seriously at fault.

On page 383 the author states that "doing the past six months I have searched carefully for a description, or even a few words of introduction to the muscular system of this strange animal, but have been able to find only a very terse discussion of the subject." He then refers to Bronn, who gives he says "only a few words to the muscles of the head." It is evident that this search was not very careful, or Mr. Davison would not have missed so important a work as Fischer's Anatomische Abhandlungen ueber die Peremibranchiaten und Derotremen 1864, where much space is devoted to the muscular system.

On p. 390 we read "Cope has greatly erred in saying that the lungs are subequal." I find on reexamination of adult specimens that the left lung is only one-tenth shorter than the right.

On p. 395 is another error, which would suggest animus, were not the author's capacity for blundering so exceptionally developed. He says "Cope has asserted that Amphiuma has only one testis, but I find paired testes extending half way from the liver to the vent." It does not appear to have occurred to Mr. Davison that I was describing one side only, and that I stated it to be single in order to distinguish it from that of Siren, where there are two on each side.

On page 403 we have a discussion of the phylogeny of Amphiuma. He gives my table of the Urodela from the "Batrachia of N. America," and then remarks. "It is evident to all phylogenists that this table presents an absurdity, since representatives of each of the five families in the direct line of descent are existing at the present time." On the contrary this naïve observation shows that Mr. Davison is a tyro in phylogeny. He does not seem to be aware that families of many vertebrata, and especially of the lower classes, often have had a long duration in geologic time. Thus in the American Oligocene occur genera of the existing families of lizards, Gerrhonotidæ and Amphisbaenidæ,

and existing families of Batrachia are known from the Miocene. But when Mr. Davison wishes to derive the immediate descent of Cœcilians from the Stegocephalia, he goes to an opposite extreme of antiquity, and, moreover, there is no resemblance whatever between the two groups. Even if the Cœcilians possess a basisphenoid as he alleges, but which I greatly doubt, this character would constitute a ground of difference from the Stegocephalia, and not resemblance.

Finally our author, in order to set forth his views of the phylogeny of the class Batrachia, copies bodily, p. 407, my diagram as published in the Batrachia of N. America, without credit, only introducing the two absurdities of deriving the Amphiumidæ and the Cœciliidæ from the Stegocephalia direct.

Mr. Davision has, in fact, adduced some new reasons in support of the proposition which I was the first to formulate, that Amphiuma is nearly related to the Cœciliidœ. So certainly have his researches with those of the Sarasins and Hay confirmed this view, that it is quite worth while to reëxamine the supposed ethmoid of the Cœcilians, and see whether there is not an agreement in this point also.

At the close of the article the author states that Dr. Scott has pointed out parallelisms in evolution of different lines of Mammalia. Dr. Scott has never claimed that his observation was original with himself, and if Mr. Davison had asked the distinguished Professor of Princeton as to this, he would have learned where and by whom this fact of phylogeny was first set forth.

Finally, the plates attached to this paper are quite unworthy of the American Journal of Morphology.—E. D. Cope.

Zoological News.—Those interested in the anatomy of the frog will find Gaupp's account of the hand and foot muscles of that animal (Anat. Anzeiger, Bd. XI, No. 7, Oct., 1895) extremely valuable, and the illustrations which accompany it are very clear. No abstract is possible.

P. J. White adds[4] *Hexanchus griseus* to the list of Selachians (*Notidanus indicus*) in which a median cartilage is inserted in the shoulder girdle. Like Haswell and Parker, he regards it as sternal in nature, and consisting of pre- and post-omosternal elements.

[3] Vitzon, Alex. N. Compte-Rend. Acad. des Sci., CXXI, 1895, p. 445.
[4] Anat. Anz., XI, 222, 1895.